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REVIEW OF "THEORY OF THE MOON'S MOTION DEDUC'D
FROM THE LAW OF UNIVERSAL GRAVITATION

By John N. Stockwell, Ph. D."

BY G. W. HILL, NAUTICAL ALMANAC OFFICE, WASH., D. C.

MR. Stockwell has, in the last fifteen or twenty years, presented to the public several memoirs on the lunar theory, of which the one, whose title is given above, is the latest and most extensive. In all of these, at least in all that have come to our notice, and we believe we have seen all, the *raison d'être* of the memoir appears to be the author's desire to controvert certain of the results arrived at by his predecessors in this subject; results, too, which have long been regarded by astronomers as definitely settled and acquired to science. But he has been singularly unfortunate in these criticisms, for there is no case, in which he has called in question the legitimacy of some procedure followed by previous investigators, but that he is himself in error, or, at least, that his objections are without foundation. This is the more to be regretted, as when the author does not come in conflict with the generally received theory, and his results agree with what was known before, he is often right in the methods employed; though it must be mentioned that the latter are sometimes unnecessarily prolix.

To follow Mr. Stockwell in his many aberrations, would make too long a story for this place, we shall be obliged to confine our attention to the above-named volume. And, even with this restriction, the space at our disposal will not allow us to write out, in full, a logical refutation of the mistakes contained in this. Nor is it necessary. For what could we do, but repeat, in a slightly varied form, perhaps, what has already been said sufficiently well by the mathematicians whom Mr. Stockwell so often controverts?

On p. XXII of his Introduction, we find the author possessed by the notion that each coordinate of the moon can be divided absolutely, and in only one way, into two parts; of the first of which we can say, this is elliptic and produced by the action of the earth, and of the second, this is perturbational and is due to the action of the sun. Now the fact is, the curve described by the moon is described under the simultaneous action of both bodies, and what belongs to each cannot be separated in the way mentioned. Our analytical expressions for the coordinates may be in such a form that it is not easy to see what they would become on making the solar force zero. For this, Mr. Stockwell appears to think that it is only necessary, in the expressions of Plana, Pontécoulant and Delaunay, to put $m=0$. And he

observes that, when this is done, there still remain some terms which he is pleased to attribute to the solar action only. Now the fact is, the putting $m=0$ implies that we have either an infinitely short month or an infinitely long year; that is, the semi-axis major of the moon's orbit is infinitely small or the semi-axis major of the sun's orbit is infinitely great. Hence, when we put $m = 0$, to be consistent, we are obliged also to put $\frac{a}{a'} = 0$: also we notice that, under these conditions, the perigee and node become stationary in the heavens. The terms which are now left in the expressions of the coordinates have only, in their arguments, multiples of the mean anomaly and the mean argument of the latitude, of the moon; and, hence, return to the same values after each revolution of the moon, since the perigee and node are fixed. Thus the curve described under these conditions is a closed one; and, when we investigate its nature, we find it to be an ellipse, and the laws of the motion of the moon in it we find to be those of elliptic motion, the central force being equivalent to that of the earth.

Mr. Stockwell's notion, that what are called the perturbational parts of the coordinates, must necessarily vanish when the disturbing force vanishes, is entirely unfounded. All that is necessary, under such a condition, is, that the resulting expressions for the coordinates should satisfy the differential equations of elliptic motion with the earth as central body. The terms which appear to have puzzled Mr. Stockwell so much, are nothing but the expression of the passage from one ellipse to another with slightly different elements. Thus falls to the ground Mr. Stockwell's startling statement "that there must be something seriously wrong in the published theories, notwithstanding their intricacy and refinement".

Mr. Stockwell, having noticed that many of the terms, which refuse to vanish when m is put equal to zero, have arguments whose motion differs from the mean motion of the moon only by quantities of the order of the disturbing force, proceeds to inquire how Laplace, Plana and Pontécoulant got such terms into their expressions. He gives his equation [3]

$$r = \frac{m^2}{m^2} \cos(it - \epsilon),$$

which seems to trouble him a great deal. So great is his dread of introducing any term, of the zero order with respect to the disturbing force, into the expressions for the coordinates, that he will not allow us to cancel the factor m^2 common to the numerator and denominator of the fraction. How this cancelling can vitiate the computation of the coefficient, the only thing we care about, one does not see. The only reason he can give for this, is, that the fraction takes the indeterminate form $\frac{0}{0}$ when $m = 0$. Now what,

ever weight such an argument might have in the case where m actually vanishes, it has no application in a lunar theory where m has some definite finite value, as is the case in the motion of the moon about the earth. But suppose that m is really zero, why need we be troubled about the result obtained by cancelling the factors? Has Mr. Stockwell never read the chapter, in our treatises on the diff. calc., on vanishing fractions? Has he, for instance, never seen the proof that $\frac{\sin x}{x} = 1$, when $x = 0$? And, moreover,

why does he neglect to notice that the term, in this case, having a period exactly equal to the period of revolution of the moon, merely expresses the change of radius vector caused by passing from one ellipse to another?

Hence we cannot accept, as true, the strange assertion that “some of the most remarkable cases of perturbation, which have hitherto been supposed to affect the moon’s motion, have no existence in nature”.

Mr. Stockwell next turns his attention to Plana’s determination of one of the terms under discussion, where the latter uses the method of variation of the elements. He follows Plana until he arrives at his equation [11], which he says agrees with what Plana has. But then follows this remarkable statement, “But this conclusion is not satisfactory”. Why a conclusion, legitimately deduced from correct principles, should be thrown aside at a mere *arbitrium*, certainly surpasses our powers of explanation. Mr. Stockwell proceeds, “I shall now show that, if we neglect the square of the disturbing force, we may suppose the elements to be constant in the differential equations, and that then we shall have $\delta e = 0$, $\delta \omega = 0$ ”. It appears from this that Mr. Stockwell does not admit there may be terms in the lunar coordinates, to the values of which we cannot obtain the lowest degree of approximation, without consenting to consider the square of the disturbing force as well as the first power of it. There are such terms, and the term under consideration is one of them. Mr. Stockwell, however, proceeds in his own way and arrives at his equation [20]. We give it here:—

$$\delta e = -\frac{21}{8} \frac{m^2}{a} e r^2 \cos(\beta_0 + \alpha v) + \frac{21}{8} \frac{m^2}{a} e r^2 \cos \beta_0.$$

Now, what is strange, Mr. Stockwell does not perceive that this equation is virtually identical with his equation [10]. The last term is constant and coalesces with the elliptic value of e , and nothing variable is left but the first term, which differs only in notation from the second member of [10]. Mr. Stockwell continues, “In the case of constant elements we shall have $\alpha = 0$, and then the value of δe will become $\delta e = 0$, [21]”. How [21] results from [20] by putting $\alpha = 0$, we must leave analysts more competent than ourselves to discover. Certainly the diff. of two infinities is not always zero.

But why insist on putting $\alpha = 0$, when we know, both from observation and our analysis, that is not actually the value of this quantity: the perigee and node are actually in motion, whatever we may say to the contrary? To forbid the perigee and node to move, is virtually to say there shall be no disturbing force, and then we are driven back on elliptic values, as Mr. Stockwell's equation [22] shows. Who does not see that, to get even the first approximation to this inequality, we must admit the movement of the perigee and node?

Our author's dislike to quantities of the order of the square of the disturbing force is remarkable; on p. XXXIV, we find him pronouncing a correction of Laplace erroneous for no other reason than that he thinks it is, or ought to be, of the order of the square of the disturbing force.

From his closing remark on this inequality, we see that Mr. Stockwell regards the coefficient of it, which is used in the present Tables, as being no less than $84''.8$ in error. But how could such a large error have escaped detection before now? We must remember that the effect of this inequality goes through all its phases in about three years, and that there is no other inequality having nearly the same period and thus capable of concealing the former. Tobias Mayer, a hundred and twenty years ago, was able to discover the small term in the moon's longitude, whose argument is the longitude of the node, simply by comparison of his rude theory with observation, and without any assistance from theory, although its coefficient does not exceed $7''$. And, recently, Sir. G. B. Airy and Prof. S. Newcomb have been able to discover small terms, with coefficients not much exceeding $1''$, in the same way. It is, therefore, simply an impossibility that such a large correction can be needed; and this consideration ought to have made Mr. Stockwell doubt the legitimacy of his mode of treating this inequality.

We next notice our author's treatment of the inequality depending on the angular distance between the lunar and solar perigees. He says, p. XXVI, that "the inequalities of long period are determined with great facility" by his equations [23] and [24]. The latter, indeed, are much simpler than any of those which have been hitherto employed to discover the value of this inequality. But truth must not be sacrificed to convenience. We cannot find any attempt at proof of these equations, either in the present volume, or elsewhere in Mr. Stockwell's writings. He seems to have adopted them quite arbitrarily. Now the theory of dimensions of units shows us that the equations [23] are inconsistent with each other. For the first member of the first is evidently of one dimension less as to the linear unit than the first member of the second; hence it is impossible that the second members can have coefficients (denoted by h) equal to each other. We

might suppose that perhaps h denoted, in general, a constant coefficient, but we are debarred from this, by observing that, in [24], it stands outside as a common factor of both terms. On turning to page 356, where Mr. Stockwell makes an application of these equations, however, we learn that the two h 's are not necessarily equal. But, this allowed, equation [24] is still out of harmony with the law of dimensions; it directs us to do something very like adding miles to square miles. It is also singular in giving an infinitely great coefficient to the inequality, when the disturbing force is infinitely small. It is almost needless for us to state that this equation is incorrect, and that the large coefficient $108''.53$, found by it for the inequality considered, is quite wrong. The reader, who wishes to see how the inequality ought to be determined, will find in *Pontecoulant*, Tom. IV, pp. 463–5, probably as brief an investigation as can be made.

The author states, p. XXVII, that he has “discovered that there is a small secular equation of the longitude arising from the oblateness of the earth” and that its value is $+ 0''.1979i^2$, i being the number of centuries elapsed since 1850. But this equation does not exist. The way the mistake arises is this:—the differential equations of the moon's motion, which are usually given, and which our author employs, suppose that the planes of reference are fixed. So that when longitude and latitude are employed, they must be referred to the fixed ecliptic and equinox of some date. Now the obliquity of the equator to a fixed ecliptic varies very slowly and proportionally to the square of the time. Peters gives as the formula, when 1800 is taken as the epoch,

$$\varepsilon_1 = \varepsilon_0 + 0''.00000735t^2.$$

This ought to have been employed instead of the author's

$$\varepsilon = \varepsilon_0 - 0''.48970t - 0''.0000012t^2.$$

And afterwards, if we desire to have the longitude and latitude referred to the mean ecliptic and equinox of date, all we have to do is to substitute the results obtained in the well-known formulæ which give the precession in longitude and latitude.

Mr. Stockwell gives, in equation [25], p. XXVIII, the value of the secular acceleration which arises from the variation of the solar eccentricity. It lies between the old value of Damoiseau and Plana and the new one of Adams and Delaunay, and does not agree with the value obtained by the author in his previous papers. Why he abandons his old value for that given here, he does not inform his readers. By elimination between eq'ns (672) and (676), p. 358, we discover that the equation employed for determining it, is

$$\frac{d.\delta v}{dt} = 2n \frac{a^2}{\mu} \left(\frac{dR}{dr} \right).$$

But this gives correctly only the first term of the coefficient, and when we wish to go as far as terms multiplied by the square and cube of the solar disturbing force, it is necessary to employ two equations, such as Prof. Cayley has given (R. A. S. Mon. Not., Vol. XXII, p. 177).

Mr. Stockwell, having calculated the coefficient of the largest inequality in the longitude, due to the figure of the earth, and finding that his result does not agree with that of Laplace, says, p. XXX, he will give "what seems to be an entirely satisfactory explanation" of this discordance. For this purpose he follows Laplace's procedure correctly and obtains his equation [43]; and remarks that the value of the right member is only one-third of that of the corresponding term in (647). Now the truth is, [43] is exact and (647) is erroneous. However the author will explain this. He says Laplace has given, in a certain chapter of his work, a general method of integration, "and that the method followed by him (Lapl.) in his investigation of the effects of the earth's oblateness does not seem to be in accordance with it". But the fact is, the chapter contains two distinct methods, the first attributes the perturbations to the coordinates, the second, to the elements; and the first is the one used by Laplace in his treatment of the present subject. Yet we find Mr. Stockwell, immediately afterward, quoting, from another chapter in Laplace, a process which is only permissible in the second method, to justify himself in putting $g = 1$ in a certain equation. Now to put $g = 1$, is to insist that the node shall not move, and thus the argument shall remain invariable. Any one can understand that a coefficient determined under such a forced condition, not in accordance with the real state of things, for we know the node does move, would very likely be widely different from that which actually has place. Consequently all the equations from [44] to [47] are quite wrong.

Mr. Stockwell now investigates a second term of Laplace, and he finds, in his equation [52], that it is the exact negative of a term in the disturbing function, which had been previously considered. He therefore fears it is some unwarranted duplicate of the latter, and calls it "the reaction of the force expended by the sun in giving motion to the moon's nodes". How such a strange designation can be applied to it, when it vanishes together with the non-sphericity of the earth, he does not inform us.

But the origin of this term is easily explained. The sun acts upon the moon in the place where the latter actually is, and not in the place where it would have been without the action of the non-sphericity. The difference of the two actions is expressed by this term. This difference was not taken into account, when equation [28], which expresses only the action of the non-sphericity, was written. Consequently it is a legitimate additional term,

which cannot be set aside, as Mr. Stockwell proposes. And his remark, “it is evident that the whole value of δv must be derived from the value of R in [5362]”, cannot be admitted.

Mr. Stockwell adds, “But, in this second part of his work, Laplace seems to have committed a grave oversight, for he has treated his equation [5372], in the construction of [5373], as though δs were constant; whereas it is a function of r and v according to [5376], which he afterwards uses in his reductions”. Now this remark has no bearing on the matter in hand; for all the partial differentiations and the integration are to be executed before the variation, expressed by the symbol δ , is taken. Thus R , to the degree of approximation adopted by Laplace, having no variables but the coordinates of the moon, we must have $\int dR = R$, and $r\left(\frac{dR}{dr}\right) = 2R$, consequently

$$3 \int dR + 2r \left(\frac{dR}{dr}\right) = 7R,$$

and, taking the variation,

$$3 \int \delta dR + 2\delta.r \left(\frac{dR}{dr}\right) = 7\delta R$$

Hence to get [5373] from [5372], we have only to multiply by 7.

Mr. Stockwell next notices the correction given by Laplace to reduce the inequality from the plane of the orbit to the plane of the ecliptic, and says, “It is apparent, however, that this correction is not required, for Laplace has shown in [923’], etc., where this subject is first treated, that this correction is of the order of the square of the disturbing force; and as terms of that order have not been considered, it is evident that the value of that correction, which he has given in [5385], is erroneous”. This statement is incorrect in every point. What Laplace has really shown in [923’] is that the reduction of the longitude from one plane to another is a quantity of the order of the square of the disturbing force, *when the inclination of the planes is a quantity of the order of the disturbing force*. But the inclination of the moon’s orbit to the ecliptic is not a quantity of the order of the disturbing force, consequently Laplace’s remark has no application in the present case. And had Mr. Stockwell taken the trouble to work out [5385] from the equations from which it is derived, he would have found it exact.

There is nothing in the present volume, nor anything in his previous publications, that substantiates the author’s assertion “that the existing theories, instead of being correct to terms of the seventh order, are really erroneous in terms of the third order”. It is a matter for regret that so much persevering labor, enthusiastically followed, for so many years, should have been given to the production of this book, since, directed in less ambitious channels, it might have brought both honor to the author and profit to science.